MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *December* 11, 1868. No. 2.

Admiral MANNERS, President, in the Chair.

Rev. C. Gribble, British Embassy, Constantinople;
Dr. G. Neumeyer, 10 Gower Street; and
David Vines, Esq., Kingswood, Bristol,

were balloted for and duly elected Fellows of the Society.

On the Preparatory Arrangements which will be necessary for efficient Observation of the Transits of Venus in the years 1874 *and* 1882. By George Biddell Airy, Astronomer Royal.

On two occasions (*Monthly Notices*, 1857, May 8, and 1864, June 10) I have called the attention of the Society to the Transits of *Venus* across the Sun's disk, which will occur in the years 1874 and 1882; and have pointed out that, for determination of the difference between the Sun's parallax and the parallax of *Venus*, the method by observation of the interval in time between Ingress and Egress at each of two stations at least, on nearly opposite parts of the Earth (on which method, exclusively, reliance was placed in the treatment of the observations of the Transit of *Venus* in 1769), fails totally for the Transit of 1874, and is embarrassed in 1882 with the difficulty of finding a proper station on the almost unknown Southern Continent.

The publication of M. Le Verrier's new Tables of *Venus*, and

of Mr. Hind's inferences from them as to the points of the Sun's limb at which the Ingress and Egress will take place in each Transit (which inferences I have in part verified), has induced me again to examine the whole subject. And, without giving up the hope of using the observation of interval between Ingress and Egress at each of two stations in 1882, I have come to the conclusion (from all the information which has reached me) that it will be unsafe to trust exclusively to the chance of securing observations on the Southern Continent; and that, while observations are by all means to be attempted in that manner, it is also very desirable to combine with them observations of the same phenomenon (at one time the Ingress, at another time the Egress), made at nearly opposite stations whose longitudes are *accurately* known, and recorded in *accurate* local time. This principle being once admitted, the Transit of 1874 is, or may be, as good for observations of that class as the Transit of 1882; and the selection of localities for the observations must be made with equal care for the two Transits.

Mr. Hind's calculations for the points of Ingress and Egress are contained in a paper in the *Comptes Rendus* of the French Academy for 1861, July 22. My valued friend has been so much accustomed, in the admirable calculations issued from his office, to describe phenomena as seen in an inverting telescope (a method of exhibition which I could wish entirely to banish) that he has inadvertently given the elements of Ingress and Egress on that supposition, but without explanation of it. Correcting for this peculiarity, the elements to be used for discussion of observation-stations are, with sufficient approximation, the following:—

Transit of Venus, 1874, *Dec.* 8. *Sun's Declination* 22° 35′ *S.*
Ingress at $14^h\ 0^m$ Greenwich Mean Solar Time.
Position on Sun's Limb 131° from South through East towards North.
Egress at $18^h\ 12^m$ Greenwich Mean Solar Time.
Position on Sun's Limb 160° from South through West towards North.

Transit of Venus, 1882, *Dec.* 6. *Sun's Declination* 22° 44′ *S.*
Ingress at $2^h\ 5^m$ Greenwich Mean Solar Time.
Position on Sun's Limb 35° from South towards East.
Egress at $8^h\ 2^m$ Greenwich Mean Solar Time.
Position on Sun's Limb 66° from South towards West.

With these data, a common terrestrial globe can be adjusted for each place, on the supposition that the Sun is vertically above the globe, and therefore that the globe-horizon defines those parts of the earth to which the phase will be visible. If a point be taken on the globe-horizon corresponding to the "Position on Sun's Limb," the terrestrial station thus indicated is that at which Parallax has its maximum effect in apparently displacing

Venus towards the Sun's centre; and the opposite station is that where the maximum apparent displacement from the Sun's centre occurs. The effect at any other station is proportional to the cosine of its distance, in arc of great circle of the Earth, from the maximum station.

The Maps which I attach to this paper have been prepared by taking advantage of the principle which I have described. They do not profess any extreme accuracy, but they will, I trust, be found abundantly accurate for the purpose for which they are prepared. All are on the same scale except the first, whose scale is $\frac{3}{4}$ that of the others. It will be remarked that the local elements, which at any station are important, are, first, the "Factor of Parallax," or the cosine of angle above mentioned, on which depends the theoretical value of the observation; second, the "Sun's elevation above the horizon," on which the chance of good observation will greatly depend. These are defined upon the maps; for the first, by the station's place among the semicircular curves; and for the second, by the station's place among the parallel lines. A point surrounded by a circle, upon the horizon-line, indicates the place at which the effect of Parallax is maximum.

It will be remarked that, in this mode of observing the Transit, the Ingress is a phenomenon complete in itself, requiring to be observed at nearly opposite sides of the Earth; namely, at one side where it is apparently accelerated by Parallax, and at another side where it is apparently retarded by Parallax. The two maps corresponding to these two tracts of observation will be placed in the bound volume, face to face. In like manner, the Egress is a phenomenon complete in itself, also requiring two districts of observation on nearly opposite sides of the Earth, different from those used for the observation of Ingress, and therefore requiring two special maps, and these in like manner will be bound face to face.

I can now proceed to discuss the individual maps with reference to the selection of stations for the observation of the several phenomena.

Transit of Venus, 1874, Dec. 8.

Map No. 1.—*Stations for observing the Ingress as accelerated by Parallax.*

Owhyhee and the neighbouring islands are excellent. The factor of parallax is about 0·92, and the Sun is at nearly two hours' elevation. There is English society at Woahoo. These islands are just within the tropics. For use of this station the absolute longitude must be accurately determined.

At the Marquesas Islands the Factor of Parallax is 0·7, and the

Sun is nearly as high as at Woahoo. Our neighbours across the Channel have, from the time of Louis XIV., taken an honourable lead in scientific enterprise of every class. I trust that we may rely on them for accurate determination of longitude at Marquesas, and for accurate observation of the Ingress in 1874.

The desert Aleutian Islands can scarcely be recommended, although the factor 0·8 for the westernmost of them, where the Sun is highest, is favourable. But it is very probable that the Russians will soon have established telegraphic communication with the mouth of the Amoor, by which its absolute longitude will be accurately determined; and though the factor is only 0·57, the Sun is 15° high, and the station will be valuable.

On the whole, if the British Government will undertake the accurate determination of longitude of Woahoo, and the careful observation of Ingress there in 1874, we may consider that good provision is made for the Accelerated Ingress.

Map No. 2.—*Stations for observing the Ingress as retarded by Parallax.*

The best station, as referred to the test of numbers, is Kerguelen's Island, where the factor is 0·91, and the Sun is 25° high. This island is emphatically known as "The Island of Desolation." I know not whether its character is so repulsive, or its utility as a zero of longitude so small, as to make our nautical authorities unwilling to determine its longitude, and to station observers there in 1874. If these difficulties are not thought too great, it will be an excellent position. At Crozet's Islands the factor 0·98 is very favourable, but the Sun is rather low (10° altitude).

The next stations in order of merit are Rodriguez, Mauritius, and Bourbon. Mauritius possesses this claim, that it will be a fairly good station, though not so good as Bourbon, in 1882; in 1874, as well as in 1882, it has this disadvantage, that the Sun will be low. If only one longitude can be determined in this chain of islands, it ought to be that of Mauritius; if two can be determined, they ought to be those of Rodriguez (for 1874) and Bourbon (for 1882).

At Madras and Bombay the factors, 0·47 and 0·44, are small; but the value of either station does not depend entirely on its simple factor, but upon the sum of its factor with the factors at the stations in Map No. 1. These two Observatories, with well-known longitudes, will prove very useful stations.

With the assistance which we may hope to receive from the British Government, we may consider the observation of the Retarded Ingress as well secured.

Map. No. 3.—*Stations for observing the Egress as accelerated by Parallax.*

Excluding from consideration the Southern Continent, as not to be entertained in our thought without the most absolute

necessity, the stations in order of merit are the Auckland Islands, Canterbury, Wellington, and Auckland, in New Zealand (factors ranging from 0·83 to 0·77), Norfolk Island (0·66), Melbourne and Sydney (0·6). I omit Chatham Island, where the Sun is rather low. The existence of the Observatories at Melbourne and Sydney makes the observation of the Accelerated Egress almost secure, although, in confirmation, I should much desire to have one station at least on the New Zealand group.

Map No. 4.—*Stations for observing the Egress as retarded by Parallax.*

The stations which are favourable for this observation are almost entirely on Russian and Turkish territories. At none of them is the factor less than 0·84; and we have, therefore, only to consider the elevation of the Sun, leaving to the national governments to estimate the facilities or difficulties depending on the locality, the climate, or the season. Any station either to the east or to the west of the Lower Caspian will have the Sun well elevated. Omsk, Orsk (whose longitude has been determined with peculiar care), Astrakhan, Erzeroum, Aleppo, Smyrna, and Alexandria, have the Sun sufficiently high. At Tobolsk, Perm, Kazan, Kharkov, Odessa, Constantinople, and Athens, the Sun will be rather low, and at Moscow it will be on the horizon. We may with the utmost confidence leave the selection of the stations, the determination of longitude, and the observation of the phenomenon, to our Russian friends. One station, however, ought specially to be considered as being, for this purpose, in British hands, namely, Alexandria. It appears not improbable that we may soon have very direct telegraphic communication with Alexandria; but failing this, I trust that no efforts will be wanting to determine accurately its longitude; a longitude which was in the survey of Admiral Smyth, and which always must be, the zero of longitude in the Levant. This being ascertained, Alexandria would probably be the best of all the stations for observation of the Retarded Egress.

Transit of Venus, 1882, Dec. 6.

First, by the method of absolute Longitudes.

Map No. 5.—*Stations for observing the Ingress as accelerated by Parallax.*

Omitting for the present all allusion to the Southern Continent, it will be seen that the best station is Kerguelen's Island, its factor being 0·98, and the Sun's elevation (12°) probably sufficient. This circumstance, in addition to its value as explained in the discussion of Map No. 2, renders it well worthy of attention. At Crozet's Islands the factor is 0·9, and the Sun's elevation 23°; abstractedly it is preferable to Kerguelen's Island,

but not in quite so great a degree as that in which Kerguelen's Island is superior in Map No. 2. The next in value are Bourbon and Mauritius, with factor about 0·78, the Sun being higher at Bourbon. On comparing these qualifications with those remarked under Map No. 2, the reasons will be evident for my recommendation that either the longitude of Mauritius or the longitudes of Bourbon and Rodriguez should be determined.

At the Cape of Good Hope the factor is about 0·62, and the observation there will be valuable.

The satisfactory observation of the Accelerated Ingress requires, however, some longitude-determinations.

Map No. 6.—*Stations for observing the Ingress as retarded by Parallax.*

Every city near the seaboard of the United States of America, and every important city of Canada, commands this phenomenon most favourably. The lowest factor is 0·95, and the smallest elevation of the Sun is 12°. The utmost reliance may be placed on the zeal of our American brethren for observing the Ingress. As great facility exists for determining the absolute longitude of any place within the range of American telegraphs (Harvard having been accurately referred to Greenwich), it is unnecessary to look further. Otherwise it might be remarked that Bermuda, Jamaica, and the West Indian Islands, and both sides of Central America, are excellent stations, but requiring determinations of longitude.

Map No. 7.—*Stations for observing the Egress as accelerated by Parallax.*

All the American stations mentioned in the last paragraph, from Halifax to New Orleans, and Bermuda and the West Indian Islands, are well situated for this observation, the factors being near 0·85, and the Sun's altitude varying from 4° at Halifax to 32° at New Orleans and Jamaica. The coast of South America also is favourable, from its union with the Isthmus to the harbour of Rio de Janeiro. It is believed that efforts have been made for exact determination, in a nautical sense, of the longitude of Rio; it may now be desirable to give to that longitude the utmost accuracy.

Map No. 8.—*Stations for observing the Egress as retarded by Parallax.*

Omitting for the present the Southern Continent, this observation will be amply secured by the Observatories of Sydney and Melbourne, where the factor is 0·96, and the Sun's elevation 12° to 14°. If, however, the longitudes of the New Zealand stations can be ascertained, they, with factor 0·8 and Sun's elevation 32°, will form a valuable addition.

Second, by the Method of interval between Ingress and Egress.

On comparing Maps Nos. 6 and 7, it will be seen that the North American localities supply, in a manner which leaves nothing to be desired, the demand for stations, at which the Ingress is retarded and the Egress accelerated, or the whole interval is diminished, by Parallax.

With these, it is necessary to combine one or more stations, at which the Ingress is accelerated and the Egress retarded, or the whole Interval is increased, by Parallax. On examining Maps Nos. 5 and 8, it will be seen that the only possible method of responding to this demand is by the selection of stations on the Antarctic Continent, in which the observation will be made when the Sun is nearly below the Pole.

In so far as the coast of the Antarctic Continent follows nearly a parallel of latitude, the best position for a station is at 7^h east longitude. The factors would be, for Ingress and Egress, about 0·95 and 0·68. The Sun would be, at each station, about three hours from the sub-polar meridian. But its elevation above the horizon would scarcely exceed 4°, and any alteration of the longitude, with the view of increasing the elevation at one phenomenon, would diminish it at the other.

Advantage may, however, be taken of the deep southern inlet discovered by Sir James Ross, to the western side of which is given the name of South Victoria. If a station can be established in latitude exceeding 72° S., it will be preferable, for observation of Ingress, to the station in 7^h longitude, and if the expedition could be pushed on to an observing place in the neighbourhood of Mounts Erebus and Terror, that position would be greatly preferable. For observation of Egress, it is manifestly far superior ; the Sun's altitude being about 27°. The factors for the two observations are respectively about 0·78 and 0·58.

The decision on the choice to be made between these two stations, and the judgment on the facility, or even the possibility, of using either of them, must rest with persons who have had some familiarity with Polar, and if possible with South Polar voyages.

In partial correction of some small inaccuracies in these remarks, it may be observed that,—

The Ingress, as viewed from the Earth's centre, is always a few minutes earlier, and the Egress always a few minutes later, than is supposed in the maps.

As affected by Parallax, the phenomenon is always retarded with ascending Sun and accelerated with descending Sun.

As referred to Apparent Solar Time, the phenomena are slightly retarded.

The only phenomena which are critically affected by these corrections are those of Maps Nos. 4 and 5, and in both the circumstances of solar elevation are rendered more favourable.

There may be a cause of uncertainty, in the observation-elements on which M. Le Verrier's Tables of *Venus* are founded, arising from the unsatisfactory form in which the observations of the planet were recorded in Bradley's time. At a critical Inferior Conjunction, when *Venus* was apparently a very large body, very deeply hollowed, it is impossible to say whether the limb was observed or whether an attempt was made to observe the centre. In my reductions of the planetary observations I inclined to the former idea. Subsequently M. Le Verrier adopted the latter. The tables are, at this time, very accurate ; and it may prove that M. Le Verrier's interpretation was correct. Any uncertainty, however, of this kind, makes it desirable to avoid observations of the Ingress or Egress very near to the horizon.

It appears, from this discussion, that the Absolute Longitudes required for the observation of the two Transits, the determination of which may be considered as a duty of the British nation, are the following :—

Alexandria.
Stations in New Zealand.
Woahoo, or some other point in the Sandwich Islands.
Kerguelen's Island, or Crozet's Islands (Kerguelen's being slightly preferable.)
Mauritius, or the two islands Rodriguez and Bourbon (which would be preferable).

I now proceed to remark on the methods by which these longitudes may be determined.

For Alexandria,—we may probably rely on the transmission of Greenwich Time, either by sending numerous chronometers in the steam ships which make regular voyages between the British ports and Alexandria, or by galvanic signals through the desired direct telegraph-communication. The saving of time and labour by the latter method would be so great that, if there is any reasonable prospect of soon forming that communication, it would be best to wait for it. The Local Time may be obtained by instruments sent for the purpose, either a Transit Instrument for meridian transits, or an Altazimuth for extra meridian zenith distances. Where the labour of computation, as for a limited object, is not worth taking into account, I prefer the Altazimuth. The largest portable instrument, with circles about sixteen inches in diameter, and with four microscopes for reading the vertical circle (two microscopes being insufficient for eliminating the effect of oval form) should be employed ; an instrument of this class now forms

part of the equipment of the Royal Observatory of Greenwich. In every expedition, however, I would send instruments of both classes. It is unnecessary to particularise clocks and chronometers, &c.

For the New Zealand stations, I should trust that connexion by chronometers might be made with Sydney and Melbourne.

For Woahoo and Kerguelen's Island, the circumstances are different. The expense of sending special steamers with chronometers, even from San Francisco to Woahoo, and from the Cape of Good Hope to Kerguelen's Island, would be very great. Here, I think, we must make use of the motions of the Moon. If a hundred transits of the Moon be taken, equally divided between the first and second limb, the probable error of the resulting longitude will be about three times the probable error of a single transit of a star ; or, with liberal allowance for every source of error, it will be below 1^s of time : this accuracy would suffice. A hundred transits might be obtained in a year with the transit instrument, or perhaps in three months by vertical transits with the altazimuth. Through this time, a competent observer must be stationed at the one point. No transmission of chronometers would be required.

The same method, I think, must be adopted for Mauritius, or for Rodriguez and Bourbon.

It would be a question for the Naval Authorities whether the determinations of Absolute Longitude should be preliminary to the observation of the Transits of *Venus*, or whether they should be made about the same time, requiring only one visit of the observer or observers to the station.

For British observation of the Transit of *Venus* in 1874, the number of sets of instruments required (each set including a transit instrument, an altazimuth, a clock, several chronometers, several telescopes, one or two observing tents) would be five (Woahoo, Kerguelen's Island, Rodriguez or Mauritius, Auckland, Alexandria.) For the Transit in 1882, the number required would be three (Kerguelen's Island, Bourbon or Mauritius, Auckland). I suppose here that the Canadian observatories are already sufficiently equipped. The instruments of the 1874 Expedition, thus set free from two stations, would be required at that of which I next speak.

For selection of an observing station on the Southern Continent, I think that it will be best to examine carefully the accounts given by Sir James Ross, and Commodore Wilkes, and their officers, and to decide on the absolute and relative probability of gaining access to the coast on December 6, in the neighbourhood of the Antarctic Circle at 7^h east longitude, or at a much higher latitude on South Victoria ; and also to judge on the possible difference in the transparency of the atmosphere. The choice of station being made, I would not recommend any reconnaissance, but I would propose that an expedition should

go direct to the selected point, in good time for the observation of the phenomenon. The season is early for south polar expeditions, and any difficulties produced by ice would probably diminish every day. A station being gained, all that is necessary in the way of subsidiary observation is, a few days' observation to give clock rate ; then the clock times only of the two phenomena will furnish all that is required.

The first action to be undertaken by the Government is, to procure the stock of instruments ; and this ought to be done without delay. An observing plant like that above mentioned is not to be obtained in haste ; and the proposed expedition might be entirely crippled by a small negligence on this point. The equipment of ships, and the selection of officers, would probably require much less time.

I have now to advert to the details of the instruments and the observations of the Ingress and Egress.

I trust that my friend Mr. Stone (First Assistant of the Royal Observatory), whose knowledge of the accounts of the Transits of *Venus* in the last century probably exceeds that of any other astronomer, and whose personal experience in the observation of the late Transit of *Mercury* is equal to that of any other, will be induced to attach to this notice his own remarks on the points to which the observer's attention ought specially to be called. We are undoubtingly of opinion that the telescopes ought to be nearly similar, with aperture perhaps of 4 to 5 inches, and used with power perhaps of 120 to 200. When there are several telescopes at one station, some variation among them may be actually advantageous. The protection of the eye from the Sun's bright light, and the neutralization of the atmospheric dispersion, require distinct consideration. From my own experience in the observation of total eclipses, and from my view of *Mercury* in the late transit, I can state positively that much is gained in clearness of outline by the use of an achromatised prismatic coloured glass, adjustible by the observer. The atmospheric dispersion enters very seriously into the question of accurate observation. The very nature of a phenomenon, which is to exhibit in the highest degree the effects of parallax, implies, that the planet must immerge or emerge near the highest or lowest part of the Sun's limb, and that the Sun must not be very high. No circumstances can be imagined in which atmospheric dispersion is more injurious. To meet this, coloured shades should be used which, as examined on a solar spectrum, absorb the ends of the spectrum, leaving the green predominant. I think it probable also that advantage would be gained by attaching to the eye-piece a dispersive prism with its acute angle upwards ; on this, however, I propose to make some experiments.

I trust that Mr. De La Rue will be induced to publish, with

this paper, his proposal in detail, for the use of photography in the register of a transit of *Venus*.

In terminating this communication, I may perhaps remark that the expedition sent into the Pacific for the observation of the Transit of *Venus* in 1769, has always been esteemed as one of the highest scientific glories of Britain in the last century; and I may be permitted to express my hope that it will be surpassed by the efforts which our country will make, with the same object, in the present century. And I close my paper with an extract from the appeal made by my illustrious predecessor, Dr. Halley, forty-five years before the earlier of the Transits then in view. The personal reflection, though applicable with less force to myself, cannot entirely escape my thought: —

"Ac sane vellem diversis in locis ejusdem Phænomeni observationes à pluribus institui, tum ad majorem adstruendam ex consensu fidem, tum ne Nubium interventu frustraretur singularis Spectator, eo spectaculo quod nescio an denuo visuri sunt hujus et subsequentis sæculi Mortales; et à quo pendet Problematis nobilissimi et aliunde inaccessi solutio certa et adæquata. Curiosis igitur syderum scrutatoribus quibus, nobis vitâ functis, hæc observanda reservantur, iterum iterumque commendamus, ut, moniti hujus nostri memores, observationi peragendæ strenue totisque viribus incumbant; iisque fausta omnia exoptamus et vovemus, præprimis ne nubili cæli importunâ obscuritate exoptatissimo spectaculo priventur; utque tandem Orbium cælestium magnitudines intra arctiores limites coercitæ in eorum gloriam famamque sempiternam cedant."

Royal Observatory, Greenwich,
December 3, 1868.

Remarks by Capt. Richards, Hydrographer to the Admiralty.

I can have no hesitation in responding to the President's invitation to offer such observations as may occur to me on the important subject which has occupied the attention of the Society this evening, viz. the approaching Transits of *Venus* over the Sun's disk on December 8, 1874, and December 6, 1882, undoubtedly, in an astronomical point of view, the most important events of the present century.

The Astronomer Royal has, in his accustomed able, lucid, and instructive manner, placed before us the objects to be attained and the means necessary to secure them; it would be quite unnecessary and out of place on my part to take up your time in supplementing these remarks of his.

We can all quite understand and sympathise with the anxiety which the Astronomer Royal must feel as to the precise action which our Government, when the time comes, will take on a question

of such vital importance to the astronomers of all nations; in ordinary affairs we do not think it necessary to look a-head so far as five years, but, as regards astronomical phenomena, concerning which there can be no uncertainty, and which do not recur but twice in a century, nothing must be left to chance which can possibly be provided for.

Mr. Airy and myself have been in communication on the subject for some months, and he has placed before the Admiralty, in the clearest manner, the arrangements which it will be necessary to make. Although I have no warrant for saying what precise steps will be taken;—indeed it would be impossible for the Admiralty to decide themselves at the present time,—yet I am justified in saying that such arrangements have been made as will insure that the whole question will be brought under their consideration at a sufficiently early period to enable the proper steps to be fully carried out; and it is impossible for any one who has followed the enlightened liberality which the Government of this country has ever adopted in such matters, to fear that Great Britain, which sent the expedition to the Pacific in 1768 for a similar purpose, will be behindhand on this occasion, when the increase in astronomical knowledge and the improvement in astronomical instruments offer such a guarantee of increased success. In regard to the absolute determination of the longitudes of the stations named by the Astronomer Royal as necessary for the observations of 1874, I cannot conceive that any difficulties or obstacles will arise, and the only immediate measures which appear necessary to be taken are in the preparation of the instruments, which undoubtedly should not be long delayed.

In regard to the selection of stations on the Antarctic Continent for the observations of 1882, this is a more serious question, and one involving considerable difficulties, though not, I apprehend, of such magnitude as is conceived by many. It would, however, certainly be necessary to make a reconnoissance in the summer of 1882, in the first place, because we know of no accessible station for a ship either in the longitude of 7 hours east, or in Victoria Land, which was noted by Sir James Ross, both of which places are mentioned by the Astronomer Royal as eligible, and in the second place, because the navigation in these regions is not open until some weeks after the time in which the transit will occur; therefore it would be necessary for the astronomers to pass a winter there, or from January, 1882, until January, 1883; this would be done either on board ship, or by landing the party, perhaps, on Possession Island, Victoria Land, on which Sir James Ross landed in 1841, or on one of the small islands to the south of it. My own opinion, looking to the uncertainty of finding a wintering station for a ship, is that the latter course would be the most feasible, and there would be little doubt of the facility of reaching one or other of these islands with a suitable steam-vessel, making Tasmania or New Zealand the base of operations; doubtless a year passed in this region would be most profitably em-

ployed in adding to our knowledge of magnetism, and various other branches of physical science.

Remarks by Capt. Toynbee.

The neighbourhood of Prince Edward's Islands, Crozet's, and Kerguelen's Islands, is, so far as my experience goes, subject to a great deal of thick weather, so that, although December may be a favourable season of the year, it would be well to have an observing station on each, in case of one or two being cloudy.

Remarks by Rear-Admiral Ommanney.

Speaking from experiences in Arctic navigation, I consider that to ensure obtaining a good position for the station of the ship on the Antarctic Continent from whence the observations are to be conducted, it is most desirable that a preliminary survey of the coast on the Antarctic Continent should be carried out before the year 1874, confining the survey more particularly to Victoria Sound, the locality for observing the transit.

Sir James Ross, in his memorable voyage, could do no more then than discover the coast line, the ships at his command were such that he could not hazard their safety by making any attempt to delineate the features of the coast, so that our knowledge of the Antarctic Continent remains where it was, of a most limited nature.

It is most important that those who will be engaged on this Expedition shall find, by previous exploration, the position for the ship to winter at, as well as to initiate the Commander and crew with some experience in the navigation of the Antarctic icy seas; without feeling our way, as it were, it would be hazarding this noble work to the risk of failure, were the expedition to proceed direct for making the transit observations with no more knowledge of the Antarctic regions than what we are now confined to.

We have not yet availed ourselves of steam-power for exploring these regions. I am of opinion that with a vessel properly constructed for the service, aided with steam-power, we could with confidence make a thorough search of the coast in Victoria Sound and the adjacent countries during the navigation season, making Van Dieman's Land the head-quarters of the Expedition.

An Expedition of this nature would be attended with many advantages for the interests of science, but most especially in training the officers for the service of the Transit Expedition when the time arrives, and knowing the nature of the position where the ship will have to take up her station, I know from experience how desirable it is that officers should have a special training for icy navigation.

I fully concur in all that has fallen from the Hydrographer of the Navy, and hope ere long to hear that operations are taking for sending out to explore the Antarctic Seas.

Remarks by Staff-Commander J. E. Davis.

Staff-Commander J. E. Davis had accompanied Sir James Ross on his southern voyage, and had himself landed at one of the suggested positions of the Astronomer Royal, and he believed there would be no difficulty whatever in again effecting a landing at the same place, viz., on Possession Island, off the coast of South Victoria, in latitude 72°; he did not agree with Admiral Ommanney that a reconnoissance and further survey of that coast was desirable, as no sign of a harbour whatever could be discovered by Captain Ross along the coast, the whole of the land being faced with ice as perpendicular as the wall of a room; indeed, so anxious was Captain Ross to find a place in which he could have wintered in the ships, that he declared he "would willingly give his right arm to find one."

With respect to the suggested station in 7^h east longitude he would observe that, although we were not certain of the position and delineation of the land in the vicinity, yet as all the land that had been seen had been described as mountainous, it was natural to suppose that such was its general character, and if so, the mountains would be immediately between the observer and the Sun; and when it was considered that the altitude of the last-named body at the times of Ingress and Egress, on either side of the pole, would only be between 4 and 5 degrees, the chances of seeing the Sun at all would be extremely problematical, and with the station on the coast of South Victoria in latitude 72° in view, at which no such difficulty existed, he thought it would be desirable to give up the position in 7^h east longitude as untenable.

With regard to the period of the season at which the Transit took place it was to be remembered that the 6th of December was so early that no ships had ever reached the Antarctic Circle by that date, and as it would be necessary to arrange the instruments, &c., preparatory to the observation, he might say that the ships ought to be on the spot at least a month before, this would be the 6th of November, a date altogether out of the question; and as the ships could not winter in the South, the party would necessarily have to be landed the year before: but with good huts he had no doubt they could pass the winter very comfortably; they would have a pleasant prospect before them, and plenty of penguins to live on.

In comparison with Kerguelen Island and the Crozet's, Capt. Davis considered the chances of observing the transit—meteorologically speaking—to be greatly in favour of South Victoria.

Capt. Davis stated that he was preparing a paper on the subject of Antarctic discovery and its connexion with the Transit of

Venus in 1882, for the Royal Geographical Society, in which he should enter more fully into the subject than he could then do.

Remarks by E. J. Stone.

In observing a Transit of *Venus* for the determination of Solar Parallax, there are two phases of the phenomena presented at the Ingress and Egress which should be accurately observed,—

(1.) The real internal contacts;
(2.) The apparent internal contacts.

It will, perhaps, conduce to clearness of explanation if we confine our attention to the phenomena presented at the egress. Similar phenomena are presented, in a reverse order, at the ingress.

The apparent diameter of the Sun is increased, and the apparent diameter of *Venus*, whilst on the Sun's disk, is decreased from the same causes which produce the spurious disk of a star in the focus of a lens. When the true limb of *Venus* falls in a line with the true limb of the Sun, the light is cut off, the spurious enlargement of the Sun's disk and encroachment on that of *Venus* is destroyed at the point of contact, and, as a consequence, a dark ligament appears to connect the apparent limbs of the Sun and *Venus*. This is the real internal contact.

The breadth of the connecting ligament rapidly increases, and, after some 18^{s}, the exact interval depending upon the circumstances of the transit and the success in first catching phase (1), the limbs of *Venus* and the Sun will appear in contact.

This is the second phase for accurate observation, the apparent internal contact. The observation of this phase is rendered more difficult by the presence of the connecting ligament. The observation, however, is of great value on account of the probable loss of phase (1) at many of the stations through atmospheric disturbance.

It will be seen from this description that the passage from the real to the apparent internal contacts is gradual. The real contacts will appear to take place instantaneously to all the observers. The description of the formation or disappearance of the connecting ligament as "instantaneous" will convey no definite information respecting the phase really observed. The required information can only be conveyed by a drawing representing, as accurately as possible, the relative breadth of the ligament, as compared with the diameter of *Venus*, when last seen at the ingress and first seen at the egress. It may be mentioned that *Venus* will appear quite within the Sun's disk at the disappearance and first appearance of the connecting ligament.

It should also be borne in mind that—if it were possible for any observer, favoured by exceptional atmospheric circumstances,

to employ extraordinary optical power and thus catch a glimpse of the connecting ligament at ingress much later and at egress much earlier than the average phase, as marked by the breadth of the ligament, caught by the rest of the observers — then such abnormal observations, however interesting for other purposes, would be absolutely injurious for the accurate determination of Solar Parallax. It is, therefore, most desirable that all the observers should employ the same optical power, which must not be fixed too high. A power of about 150 would, perhaps, meet all requirements. I would, therefore, suggest for consideration,—

1. That real internal contacts be observed.
2. That apparent internal contacts be observed.
3. That a magnifying power of about 150 be employed.
4. That all the observations should be accompanied by drawings representing, as accurately as possible, the *relative breadth of the connecting ligament as compared with the diameter of Venus*, corresponding to the time of each observation. At the real internal contact, at ingress, the breadth of the ligament when last seen should be represented.
5. Of course full notes would be given of the circumstances under which the observations were made.

On the Observation of the Transits of Venus by means of Photography. By Warren De La Rue, Esq.

The Astronomer Royal having intimated to me his intention of communicating to the Society a paper on the Transits of *Venus* of 1874 and 1882, and having invited me to make any suggestion which may have occurred to me on the means which Photography offers for their observation, I venture to lay the following statement before the Society: premising, however, that I do not suggest that photographic observations should displace eye observations; on the contrary, I think that both eye and photographic observations ought to be made.

It will be recollected that in 1860 I made photographic observations of the phenomena of totality, and also of the different phases of the eclipse of the Sun before and after that of centrality; the photographs of the totality and of partial phases were measured by means of a micrometer which I designed for that purpose,* and the results tabulated† and discussed.

From the discussion of the measurements of the partial phases, the angular position of the Moon's centre in relation to the Sun's centre and the distances of the Sun and Moon's centres were derived and compared with these elements calculated by Mr. Farley.‡

* This is described and represented in *Phil. Trans.*, 1862, pp. 373–374,
† Ib. p. 383. ‡ Ib. p. 387, Table III. columns.

On reference to these results, it will be seen that the observed and calculated positions agree remarkably well,* and that the deduced quantity for the nearest approach of the Sun and Moon's centres only differs by $\frac{9}{100}$ths of a second of arc from that calculated by Mr. Farley and 1.2 second from that calculated by Mr. Carrington.

The conditions which transits of *Venus* offer for the determination of the relative position of the Sun and planet's centres are more advantageous than those presented by solar eclipses, inasmuch that it is far more easy to measure directly the distances between the centre of the disk of the Sun and that of the image of the planet upon it, than it is to measure the distances between the peripheries of the Sun and Moon,† or the angular opening of the cusps‡ of the partially eclipsed Sun. And in transits of *Venus* any error of observation would not affect the final result nearly so much as in solar eclipses; for example, in the transits of 1874 and 1882, an error of 1″ in the measurement would, for the maximum displacement, give an error of only 0″·185 in the deduced Solar Parallax.

Moreover, it may be observed that in photographic records it is by no means important to catch exactly the phases of contact, as two photographs obtained at a sufficient interval afford the means of calculating to a great degree of refinement and of tracing the path of the planet, which, for the conditions of the problem, may be considered to be a straight line between the two positions recorded.

Nor is it in any way essential, as it is with eye observations, that favourable conditions should exist for retarding the period of contact at one station and of accelerating it at another, because the chords representing the planet's path can be derived from photographic records, with as much accuracy under what would be considered unfavourable conditions as under favourable conditions for eye observations, for the length of the chords need not be directly considered in determining the nearest approach of the Sun and planet's centres.

During the duration of the transit it would be possible in a clear state of the atmosphere to obtain a series of photographs at intervals of two or three minutes, and any or all of these would be available for comparison with the records obtained at all the stations selected.

The epoch of each photographic record is determinable with the utmost accuracy, 1st, because the time of exposure is not more than the $\frac{1}{50}$ or the $\frac{1}{100}$ of a second, and 2nd, because the instantaneous slide, as it flashes before the secondary lens, affords an audible signal§ by striking against a stop a small fraction of second after it has shut off the image of the Sun. This interval might be determined by experiment and taken into account.

* *Phil. Trans.* 1862, p. 393, Table IV.
† Ib. p. 383, Table I.
‡ Ib. p. 55, Table III.
§ Ib. p. 364.

In figures 1 and 2 is represented the solar disk of the dimensions of the photographs which would be given by the Kew Photoheliograph at the epochs of 1874 and 1882, under the same conditions of adjustment as existed during the eclipse observations of 1860; on this occasion $\frac{1}{1000}$th of an inch represented 0″·496 second of arc;* the images are direct, as given by that instrument.

The following data, part of which are derived from Mr. Hind's paper in the *Comptes Rendus* for July 22, 1861, on the circumstances of the Transits of *Venus* for 1874 and 1882, have been used in setting out the diagrams.

1874.

⊙ Semi-diameter (Le Verrier)	16′ 14″·98
⊙ Inclination towards East of Axis to a Meridian drawn through Sun's centre	12° 49′
Heliographic Latitude of Earth S.	22′
♀ Semi-diameter	31″·41
Ingress, Ext. Contact 49°·4 N. towards E.	
Egress, Ext. Contact 20° N. towards W.	
♀ Horizontal Parallax	32″·44
⊙ ,, ,, (by Encke)	8″·71

1882.

⊙ Semi-diameter (Le Verrier)	16′ 14″·64
⊙ Inclination towards East of Axis to a Meridian drawn through Sun's centre	13° 52′
Heliographic Latitude of Earth N.	12′
♀ Semi-diameter	31″·40
Ingress, Ext. Contact 145°·3 N. towards East	
Egress, Ext. Contact 113°·9 N. towards West	
♀ Horizontal Parallax	32″·43
⊙ ,, ,, (by Encke)	8″·71

In the Kew Photoheliograph the solar disk would, at the epoch of the transit of 1874, have a semi-diameter of 1965·8 thousandths of an inch (a diameter of nearly four inches), *Venus* a semi-diameter of 63·33 of these units, and the parallax of *Venus* referred to the Sun would be represented by 47·85 of these units; the maximum possible displacement being 95·7 units, or nearly $\frac{1}{10}$th of an inch. In 1882 the Sun's semi-diameter would

* *Phil. Trans.* 1862, p. 383, Table I.

be 1964·9 units; that of *Venus* 63·31 units; the parallax of *Venus* referred to the Sun 47·82 units; the maximum possible displacement 95·6 units.

When the photographs have been secured, the measurements by means of the micrometer, which would have to be performed, consist in determinations of the Sun's semi-diameter, in units of the arbitrary scale of thousandths of an inch, the angle of position of *a*, *b*, *d*, &c., and the distances *a c*, *b c*, *d c*, figs. 1 and 2; with these data the portion of the chord *a b*, *b d*, &c., could be calculated. The measurements by means of the micrometer described in the *Phil. Trans.* 1862, pp. 373-374, can be obtained to the $\frac{1}{2000}$th of an inch (0″·25), and the position angles to one or two seconds of arc. For each photograph measurements made at different times are remarkably accordant; the greatest difference between the semi-diameter of the Sun of the several eclipse pictures of 1860 was $\frac{9}{1000}$ths of an inch, or about 4″·5; but, on taking the mean of measurements of forty-five photographs by two different methods, the difference was only $\frac{1\cdot5}{1000}$, or about 0″·75. I am inclined to believe that the distance *a b*, *a c*, could be ascertained to within 1″, by means of a few pictures, and possibly to 0″·25, if a sufficient number of photographs were obtained.

Since 1860 means have been devised of ascertaining the optical distortion of the photographic pictures, and, in a paper* recently sent in to the Royal Society by myself, Mr. Balfour Stewart, and Mr. Loewy, this has been treated of; if this optical distortion had been ascertained for the photographs of the eclipse, 1860,† it is certain that even more accurate results would have been obtained, because a part of the small discrepancies in the measurements arose out of the circumstance of the centre of some of the pictures not being in the optical axis of the instrument. In the eclipse pictures the whole disk of the Sun was not, however, visible, but in the transit pictures the whole disks of the Sun and *Venus* would be seen, and the determination of the requisite data, consequently, far more accurate. By photographing a suitable scale of equal parts placed at about a mile or two miles from the heliograph, a picture would be obtained in which different equal portions of the scale would occupy different lengths; by measuring the divisions by means of the micrometer, the effect of optical distortion could be ascertained to a great nicety.

Such a scale of equal parts, if erected at a distance of two miles, would have to be about 110 feet long, in order that its image might be somewhat longer than that of the diameter of the Sun picture. It could be made by fixing two horizontal rails of

* Now being printed in the *Phil. Trans.*

† It is not possible to do so now on account of the relative adjustment of the object-glass and secondary lens having been disturbed after the observations had been made.

wood, one over the other, on vertical posts, so that their outer edges would be distant about 3 feet. On these horizontal rails plates of zinc, 2 feet wide and 3 feet long, would have to be fastened so as to leave an interval of exactly 2 feet between two adjacent plates. These plates could all be made of exactly the same width by clamping the whole series together and planing their edges in a planing machine; in fixing them on the horizontal rails, one of the plates could be used temporarily as a template to govern the interval between a plate previously screwed down and one about to be fixed. The horizontal bars would be strengthened here and there by cross bars of wood behind the plates and by cross tension rods properly distributed. To facilitate the depiction of the scale, it should be placed on the summit of a low hill or, by preference, near the sea-shore, in order that the light of the sky might shine through the intervals between the plates.

The focus of the object-glass of the Kew Heliograph, for an object situated at a distance of two miles, would be about $\frac{8}{100}$ths of an inch longer than that for parallel rays; this elongation of the focus produces a little indistinctness in the pictures, but does not prevent measurements of them being made. Before finally fixing the position of the secondary (or enlarging) lens for Sun pictures, sharply defined pictures of the scale could be procured by adjusting the focus accurately for the scale of equal parts; subsequently other pictures of the scale could be procured after the focus had been adjusted and fixed for Sun pictures. This double set of pictures would afford all the requisite data for ascertaining the correction for optical distortion; and, the exact distance of the scale of equal parts being known, for obtaining the value in arc for the divisions of the micrometer.

Fears have been expressed that the collodion, in drying, becomes distorted; experiments, however, in 1860–61 have demonstrated that the shrinkage is only in the direction of the thickness.* But, as in the case of the Solar Parallax no refinement of correction ought to be neglected; it would be quite easy to ascertain once more whether any distortion does take place, by taking photographs on glass plates on which lines about $\frac{1}{4}$ of an inch distant had been previously etched, the collodion, which should be rendered purposely contaminated with particles in suspension, should be poured on the ruled sides to avoid parallax:—after all the operations of photography the film would have to be examined from the back, and the position of certain impurities with reference to the ruled lines noted whilst the collodion was wet, and after it had dried.

Lastly, on looking at the charts given in the May number of 1857, and in the June number of 1864, in Mr. Airy's valuable papers, it appears that, at all events, six stations could be selected favourable for photographic records: I would propose that six

* *Phil. Trans.* 1862, p. 36.

precisely similar instruments should be prepared and mounted equatoreally, but without circles, or a driving clock, as these are quite unnecessary. The optical distortion of these instruments could be determined beforehand, and no further experiment would be necessary, as all the optical parts must be rigidly fixed; it would only be necessary to make a provision for the rotation of the telescope in a cradle on the declination axis for the object of adjusting the cross wires in a position of 45° to a meridian passing through the Sun's centre; in other respects the mounting would be of the simplest kind.

No difficulties exist in photographing a transit of *Venus;* the operations are quite the same as those practised daily at the Kew Observatory; no strain on the nerves would occur, as in the anxiety consequent on the desire of rendering available every moment of the short duration of a solar eclipse. All the operations could be conducted with that calm so essential for such a problem as the determination of the Solar Parallax, and I feel confidence in recommending that timely steps should be taken to secure photographic records of the Transits of *Venus* in 1874 and 1882.

Ephemeris for Observations of the Physical Features of the Planet Mars during the Opposition of 1869. By A. Marth, Esq.

(*Communicated by R. S. Newall, Esq.*)

The following Ephemeris may probably be of service to those observers who are engaged upon observations of *Mars.* It is computed on the assumption that the axis of rotation of the planet is directed to the point R.A. 317° 51′, P.D. 39° 46′, and that the time of rotation is = $24^h\ 37^m\ 22^s{\cdot}62$ mean time. The Ephemeris gives for 8h Greenwich Sidereal Time of every 5th day the values of—

L, the Areographic Longitude of the Apparent Centre of the Planet's Disk, reckoned from an arbitrary First Meridian to the West.

B, the Aerographic Latitude of the Centre, or the Elevation of the Earth above the plane of Mars' Equator.

P, the Angle of Position of the visible Pole at the Centre.

Q, ,, ,, ,, greatest Phase.

q, the Breadth of the greatest Phase } the Diameter of Mars at the Distance
d, the Diameter of the Disk } 1 being assumed = 9″·328.

8h Gr. Sid. T.	L.		B.	P.	Q.	*q.*	*d.*
1869.	°	°	° ′	° ′	° ′	″	″
Jan. 1	260·0	−1·1	24 21 N.	13 56	289 4	0·66	10·89
6	209·7	−0·7	24 11	13 57	288 24	·59	11·36
11	159·8	−0·4	23 57	13 46	287 29	·50	11·83
16	110·2	0·0	23 41	13 22	286 14	·40	12·29
21	61·0	+0·3	23 22	12 44	284 26	·30	12·72
26	12·1	+0·5	23 2	11 53	281 47	·20	13·10
31	323·4	+0·8	22 40	10 51	277 23	·12	13·41
Feb. 5	275·0	+0·9	22 16	9 40	268 21	·05	13·64
10	226·7	+1·0	21 52	8 22	240 59	·01	13·76
15	178·5	+0·9	21 27	7 1	164 49	·01	13·76
20	130·2	+0·7	21 4	5 40	132 55	·05	13·66
25	81·7	+0·5	20 44	4 24	122 54	·11	13·44
Mar. 2	33·0	+0·3	20 26	3 17	118 5	·19	13·14
7	344·1	−0·1	20 14	2 20	115 9	·29	12·77
12	294·8	−0·4	20 7	1 34	113 10	·39	12·35
17	245·2	−0·7	20 6	1 2	111 48	·48	11·90
22	195·3	−1·1	20 11	0 44	110 45	·58	11·44
27	145·0	−1·4	20 21	0 39	110 1	·65	10·97
Apr. 1	94·4		20 37 N.	0 45	109 32	0·71	10·52

Increase of L for other Hours and Days.

Gr. Sid. T.	0	1	2	3	4
h	°	°	°	°	°
4	301·6	291·8	282·0	272·1	262·3
5	316·2	306·4	296·6	286·7	276·9
6	330·8	321·0	311·1	301·3	291·5
7	345·4	335·6	325·7	315·9	306·1
8	0·0	350·2	340·3	330·5	320·6
9	14·6	4·8	354·9	345·1	335·2
10	29·2	19·3	9·5	359·7	349·8
11	43·8	33·9	24·1	14·3	4·4
12	58·4	48·5	38·7	28·8	19·0
13	73·0	63·1	53·3	43·4	33·6
14	87·5	77·7	67·9	58·0	48·2
15	102·1	92·3	82·5	72·6	62·8
16	116·7	106·9	97·0	87·2	77·4

Hourly Increase, 14°·59.

This is the increase for the average difference of the successive values of L, which is = 1750°·80. The deviations from this average, which depend on the variation of the planet's apparent

motion and distance, are given above as differences between the values of L, and must be allowed for where needed.

To diminish the existing uncertainty of the direction of the axis of rotation, trustworthy measures of the angles of position of some well-defined spot near the pole with respect to the centre of the disk are required. The spring equinox for the northern hemisphere of *Mars* has occurred, 1868, Sept. 28, and the summer solution will take place, 1869, April 14. It seems, therefore, not unlikely that, during the next months, the white spot (supposed to be snow) near the pole will offer a good object for observation. But, to get rid of the assumption that its centre occupies a constant position on the ball of the planet, and of the still greater assumption that it coincides with the pole itself, it is necessary that trustworthy angles of position should not only be observed on a sufficient number of nights, but also that, if practicable, two or three sets of observations be taken each night at intervals of some hours.

Though liable to some uncertainty, the direction of the axis and the time of rotation are sufficiently well known for the purpose of placing the drawings of *Mars*, which have been made during the last ten or twenty years, in their proper order. A list of the sketches known to us, arranged in the order of the areographical longitudes and latitudes of their centres, will be communicated when it can be given with some completeness. At present, probably several series of published sketches are not yet at our disposal. Of unpublished drawings there are very likely some series which may deserve to be at least consulted. Several of them are probably in the library of the Royal Astronomical Society. Observers who desire to have their sketches inserted in the proposed list will, perhaps, consider it worth while to communicate to us (at Ferndene, Gateshead), if not copies or tracings, at least the approximate times (if possible within a few minutes) to which their drawings refer.

A statement of the values of B and P for the times of the last half-dozen oppositions may, perhaps, be of interest to some readers.

		B.	P.
		° ′	° ′
Opposition 1856	April 2	25 49 N.	33 35
58	May 15	12 4 N.	40 43
60	July 17	14 22 S.	189 26
62	Oct. 5	22 40 S.	145 17
64	Nov. 30	6 25 S.	142 40
67	Jan. 10	10 38 N.	343 54

Description of Drawings of Mars. By J. Joynson, Esq.

The accompanying sheets are lithographed copies of a series of diagrams already sent to the Society. They show in consecutive order the appearances presented by the planet during one complete rotation, at each opposition, from 1862 to 1867. They have been lithographed for the purpose of sending a copy of each to any Member of the Society or others interested who may apply for them, and at the same time undertake to send me a similar series, to be taken by the applicant during the coming opposition; as, by this means, it is hoped that something near a correct delineation of the planet during one complete rotation will be obtained. It is earnestly requested that, as far as possible, every observer will obtain the appearances during the whole rotation, and so avoid the mistake that has been made of supposing that the same appearance was presented by two different parts of the planet. It is also particularly desired that no approach to fancy sketches may be made, or any attempt permitted in the way of striking drawings; but that they be strictly confined to the appearance presented by the planet at the time noted at the foot of each drawing. The aperture of the telescope and the power used should be given.

The plan adopted by the writer was to commence the series as soon as the planet presented well-defined markings on the disk, and then make a drawing about every forty minutes. By this means four or five drawings were made; and by doing the same on the next evening, commencing about the same time, the observer would soon see that the drawings arranged themselves somewhat in this order;—

	st	nd	rd	th	th
1st Night's Drawings	1	2	3	4	5
2nd ,,	2	3	4	5	
3rd ,,	3	4	5		
4th ,,	4	5			

That is, he would get about one aspect sooner each night. What is seen, say at 7 o'clock on the first evening, is not seen on the next until $7^h\ 37^m$, and on the following until $8^h\ 15^m$, varying about 37 minutes each succeeding night.

However the drawings are originally made, — that is, with reference to the Vertex at the Pole of the Earth,—it will be desirable to place them all in the final drawing with the pole of the planet in one direction.

Waterloo, near Liverpool, October 2, 1868.

The Transit of Mercury. By John Browning, F.R.A.S.

On the morning of November 5th the Sun rose to view at my small telescope about $8^h\ 15^m$. At this time *Mercury* was at once seen as a very dark purplish-grey spot on the solar disk. The limb of the planet was ill defined, perceptibly darkest towards the centre, and surrounded by an aureola slightly brighter than the solar disk. Near the centre of the planet appeared two bright grey spots close together.

The spot to the N.W. seemed to be much the most conspicuous, and to be elongated, in the direction of the smaller spot. Possibly the faint spot was produced by the boiling of the image, which was sometimes very bad.

Mr. Pritchard has kindly informed me that the shadow of a black disk is a white spot. Possibly this may prove the explanation of these otherwise unaccountable appearances.

When the limb of the planet was still distant nearly one-third of its diameter from the Sun's limb, the limb of the planet seemed drawn out towards the Sun's limb. About 23^s after this the contact seemed complete.

For this observation I used a silvered-glass reflector of $4\frac{1}{2}$-in. diameter, and $5\frac{1}{2}$ feet focus, made for me by Mr. With. The mirror had been tested up to 700 on stars. The Sun being low I did not use a solar eye-piece, but simply modified the light with a sliding wedge of dark neutral tint-glass corrected for refraction.

Of the three drawings I have the honour of exhibiting before the Society the first represents the planet and shows the aureola and the two white spots. The other two show the appearance of the planet at the times of real and apparent contact with the Sun's limb.

As I did not see either the aureola or the white spots towards the time of contact, I have omitted them in the figures which represent the contact.

The Transit of Mercury. By C. G. Talmage, Esq.

The transit of *Mercury* was well seen here. At sunrise the sky was perfectly cloudless, and remained so until nearly noon. When I first directed the telescope to the Sun at $19^h\ 50^m$ M.T. the limbs were boiling violently, but by $20^h\ 30^m$ the definition was greatly improved. From the first the limbs of *Mercury* were much better defined than those of the Sun. I watched very carefully for any apparent distortion of the limbs of *Mercury*, but saw none. The time of internal first contact I consider better than the time of last contact.

	Egress. h m	h m
First contact	21 0	12 38
Last contact	21 3	5 70

I used the full aperture of 10 inches, with a diagonal eye-piece. Power 200.

Mr. J. G. Barclay's Observatory,
Leyton, Essex.

The Transit of Mercury. By G. Knott, Esq.

Although the morning of November 5th was fine, definition in the telescope was so disturbed that the transit of *Mercury* was not well observed at this station. With an aperture of $2\frac{3}{4}$-in. and a negative eye-piece magnifying 89 times, I observed the egress as follows,—

	h m s	
First contact, Nov. 4	21 0 25·9	G.M.T.
Last contact	21 2 32·6	

both results being very uncertain. With the full aperture, $7\frac{1}{2}$-in. and a Dawes' solar eye-piece with powers 95 and 145, I carefully scrutinised the planet during intervals of tolerable definition. At times I fancied that I saw traces of a narrow penumbra surrounding the body of the planet, but when best seen the disk appeared to be uniformly black, and the bright spot described by some observers, though looked for, was not seen. In a transient interval of pretty steady definition some few seconds before first contact, I caught a glimpse of a fine black ligature connecting the planet with the Sun's limb. The "boiling" of the limb was so troublesome that the observed times of contact may be some seconds in error.

Woodcroft Observatory, Cuckfield,
December 10, 1868.

Description of a new Chronograph Pen. By R. L. J. Ellery, Esq., Director of the Observatory of Melbourne.

The drawing, which gives the full size of the pen, is in the woodcut reduced to the scale of one-third.

Fig. 1. *a* is the reservoir of glass, on which a brass cover, *b*, *b'*, is cemented. The syphon, *e*, *e*, *e'*, is made of spirit thermometer tube, about 0·2 inch diameter, with a bore of 0·05 inch, and is bent as shown, so that one leg reaches nearly to the bottom of the reservoir; it is fastened to the cover by a joint like a gas-union, one part of which is cemented to the tube, another part is soldered into the cover, and the clamp screw M draws the two parts together air-tight; the length of the tube will depend on the size of the barrel, distance of electro-magnet, &c. At *e'* the tube is bent at a convenient angle, and the end ground square,

on this end a tube carrying a screw-thread is cemented, into which the nib is clamped as shown at *n n*. To support the tube an arm, *h h*, of thin brass projects from the reservoir cover, and ends in a notch at *o*, in which the tube lies, and in which it is firmly retained by means of the collar and the screw, *f*, *g*. The air tube, *c d*, is a piece of tube like the syphon, but reaching to

Fig 1

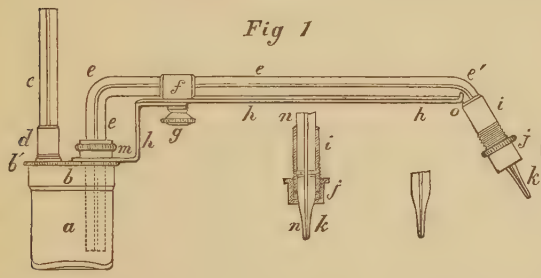
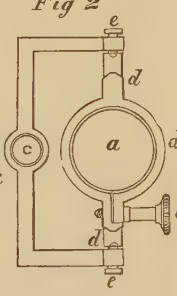


Fig 2



just within the cover. The cover *b′* itself unscrews from the collar *b*, which is permanently cemented to the reservoir. The nib is made of same sized tube as the syphon by drawing out in a blow-pipe flame ; some skill, however, is required to reduce the bore gradually towards the point, and at the same time retain sufficient glass around it. The nibs are ground square at the thick end, and cut off to the required length or to the required fineness of bore, and then ground to a rounded point on a stone or with fine emery, and slightly polished on Arkansas stone or with rouge to take away any asperities likely to catch the fibres of paper. Sketch *n n* shows how it is fitted on ; a perforated washer of sheet India rubber being placed between the ends of the tube and nib. The fluid used is a strong solution of Judson's solid aniline blue in boiling distilled water. To fill the pen the cover is unscrewed, or *d* taken out, and the requisite quantity of fluid put in; screwing the cover on air-tight and blowing into the air-tube C forces the ink through the syphon into the nib, from whence it oozes in a drop. The drop being removed and the pen adjusted in a suitable holder on the armature lever of the electro magnet, with the nib resting on the revolving barrel, the ink will flow as freely or as slowly as required, by altering the "head," which is readily done by lowering or raising the reservoir in the clip or holder, of course the pressure of the pen on the barrel or paper must be regulated by suitable springs or counterpoises.

The advantages of this pen are that any sediment that may occur in the fluid falls to the bottom of the reservoir, and is never likely to reach the fine bore of the nib. When once filled it can be used day after day till empty; the flow, if stopped by drying at the nib, can be usually re-established by rubbing with the moistened finger, or, at the worst, by removal of the nib. The flow can be regulated by raising or lowering the reservoir in its

clip, and very fine or very strong lines obtained by using nibs with finer or coarser bores.

Fig. 2 represents the arrangement by which the pen is attached to the electro-magnet armature. C is the armature axis; *d d d*, the pen-holder suspended between two centre screws *e e*.

The reservoir is clamped in the ring *a*, which is lined with a piece of leather and clamped by screw *b*.

Observations of the Meteoric Shower of November 13–14, 1868, *made at the Glasgow Observatory.* By Prof. Grant.

During the earlier part of the night the sky was so overspread with dense vapours that nothing could be seen. About $15^h\ 30^m$ the clouds commenced slowly to disperse, and through the openings between them a meteor might now and then be seen shooting across the heavens. The state of the sky continued to improve till about $16^h\ 30^m$, when it finally became pretty clear in all directions. It now became evident that a shower of meteors was in progress ; nor did it require long time to be convinced that the phenomenon was no other than an apparition of the great November shower which had been so conspicuous during the two or three previous years. In every instance the course of the meteors was found, when traced back, to emanate from the constellation *Leo*, the radiant appearing to coincide very nearly with the middle point of a line joining γ and ε of that constellation. At $16^h\ 20^m$ the number of meteors visible amounted to two or three in a minute. In their general features they presented no essential difference from the meteors observed on the occasion of the memorable apparition of November 13–14, 1866. Their colour was white, mingled in some instance with a slight tinge of red. They generally were accompanied by trains which lasted two or three seconds. I failed, however, to discover any trace of the beautiful green which formed so interesting a feature of many of the meteors of November 1866. So long as they were attached to the meteors the trains appeared white, but on breaking up they assumed a greyish aspect slightly tinged with red. In three or four instances I remarked that the *débris* of the train continued visible in the heavens for two or three minutes after the disappearance of the meteor. Some of the meteors were of considerable size, three or four of them far exceeding *Jupiter* in brightness, but not equalling the planet *Venus*, which was shining with intense brilliancy in the east, and formed an excellent standard of comparison for estimating the brightness of the larger meteors. It was interesting to note the excessively short trains of the meteors in the immediate vicinity of the radiant point. I failed, however, to perceive any instance of a meteor which appeared absolutely motionless during the time of its visibility.

The shower continued sensibly to increase from $16^h\ 30^m$ till $16^h\ 56^m$, and as it appeared very desirable to endeavour to ascer-

tain the time of its maximum, I now proceeded, in conjunction with Mr. John McKinnel, the junior assistant, to count the number of meteors which might become subsequently visible. The results are given underneath. It would appear from them that the maximum of the shower occurred at $17^h\ 15^m$.

Shortly after 18^h it became cloudy, and we ceased to obtain any further observations of the phenomenon, but although the greatest intensity of the shower seemed to have already passed, it was very evident that it had not yet wholly expended itself.

Time. G.M.T.	Number of Meteors Counted.
h m to h m	
16 56 to 16 57	1
16 57 16 58	2
16 58 16 59	6
16 59 17 0	8
17 0 17 1	5
17 1 17 2	5
17 2 17 3	6
17 3 17 4	2
17 4 17 5	7
17 5 17 6	8
17 6 17 7	7
17 7 17 8	5
17 8 17 9	2
17 9 17 10	4
17 10 17 11	9
17 11 17 12	6
17 12 17 13	11
17 13 17 14	4
17 14 17 15	3
17 15 17 16	3
17 16 17 17	3
17 17 17 18	5
17 18 17 19	2
17 19 17 20	1
17 20 17 21	5
17 21 17 22	4
17 22 17 23	6
17 23 17 24	4
17 24 17 25	3
17 25 17 26	3
17 26 17 27	7
17 27 17 28	3
17 28 17 29	5
17 29 17 30	3
17 30 17 31	3

Time. G.T.M.	Number of Meteors Counted.
h m to h m	
17 31 to 17 32	2
17 32 17 33	3
17 33 17 32	4
17 34 17 35	5
17 35 17 36	4
17 36 17 37	1
17 37 17 38	0
17 38 17 39	4
17 39 17 40	4
17 40 17 41	3
17 41 17 42	4
17 42 17 43	5
17 43 17 44	3
17 44 17 45	1
17 45 17 46	1
17 46 17 47	2
17 47 17 48	3
17 48 17 49	1
17 49 17 50	3
17 50 17 51	2
17 51 17 52	2
17 52 17 53	2
17 53 17 54	2
17 54 17 55	3
17 55 17 56	4
17 56 17 57	5
17 57 17 58	0
17 58 17 59	1
17 59 18 0	0
18 0 18 1	2
18 1 18 2	6
18 2 18 3	4
18 3 18 4	3
18 4 18 5	2
18 5 18 6	4

It appears from these results that 256 meteors were counted during the interval of one hour and ten minutes included between $16^h\ 56^m$ and $18^h\ 6^m$. There can be no doubt, however, that the number really visible was considerably greater than this. It was certainly twice, and in all probability was three times as great. We may, therefore, infer that during the interval in question as many as seven or eight hundred meteors were visible.

In order to obtain a clearer view of the time of maximum I have arranged in groups of ten minutes the interval during which the meteors were counted, and have inserted the number of meteors counted in each of the subdivided intervals thus:—

			Meteors Counted.
	h m	h m	
From	16 56 to	17 6 ...	50
	17 6	17 16 ...	54
	17 16	17 26 ...	36
	17 26	17 36 ...	39
	17 36	17 46 ...	26
	17 46	17 56 ...	24
	17 56	18 6 ...	27

It would appear from a comparison of these results and the general observations of the phenomenon with the corresponding observations of 1866 that the shower was much inferior in density to that of the great November shower of 1866. We have reason, however, to believe that it was characterised by a more uniform structure.

The Observatory,
Glasgow, Dec. 10, 1868.

Meteors of November 13–14, 1868. By Prof. D. Kirkwood.

The shower of November meteors has been this year unexpectedly brilliant. Unfortunately I was unable to watch the display; but during three hours on the morning of the 13th, Prof. T. A. Wylie, D.D., observed 165. A considerable proportion of these were unconformable. The number, however, radiating from *Leo* was sufficient to indicate the commencement of the shower. Remembering that Humboldt, in 1799, had seen a number of meteors after sunrise, I took a position at 8 o'clock A.M., on the west side of a building (in the shade) and watched the vicinity of the Radiant, in the hope of observing some of the largest meteors. Glimpses of five or six were, as I thought, actually obtained, when, in order to guard against optical deception, I called Mr. Allison Maxwell, a tutor in the State University, who watched about fifteen minutes. This gentleman re-

ported that he saw *one* beyond doubt, and three others less certainly. I mention the case as being the first, since the commencement of the present century, in which meteors of the November stream have been (probably) seen in the daytime.

On the night of the 13th, a committee of the senior class in the University kept watch from 11 o'clock P.M., till 4.15 A.M., when, having counted 2500, they adjourned. The maximum was about 3.30; 900 meteors having been counted during the 45 minutes immediately preceding. Many of the meteors were very brilliant, leaving long trains, some of which continued visible several minutes. Three or four were observed to explode, or separate into several fragments. No sound, however, was in any case perceived.

At $4^h\,55^m$ (40 minutes after the committee at the University adjourned) Prof. Wylie commenced observing at his residence, and continued his watch till $6^h\,11^m$, counting 780 in $1^h\,16^m$. The whole number therefore actually counted in $6^h\,31^m$ was 3280. A portion of the heavens was at times overcast with clouds; especially was this the case shortly after 4 o'clock, when the committee of students adjourned.

At frequent intervals throughout the night a lull occurred in the display; while at other times for a few seconds the meteors were so numerous that they could scarcely be counted.

A remarkable feature in the shower of the present year is its duration. As seen in Europe in 1866, and in this country in 1867, the display was limited to three or four hours. On the present occasion, however, it commenced on the night of the 12th, and had not ceased at daylight on the morning of the 14th. This would indicate considerable irregularity in the thickness of the stream.

Bloomington, Indiana, Nov. 14, 1868.

The Lunar Crater Linné. By W. R. Birt, Esq.

Having been favoured by Mr. Huggins, Capt. Noble, and Herr Von Mädler, with drawings of *Linné* as it appeared on June 26th, Aug. 26th, and Oct. 22nd, 1868, which in many respects perfectly agree with views which I obtained on the 26th of June with the Crossley Equatorial of 7·3-inch aperture, and on the 22nd of Oct. with Mr. Barclay's 10-inch refractor, I have endeavoured from them to construct an approximate section of *Linné* as it existed on Aug. 26. The drawing of this date was made by Tacchini of Palermo, and kindly forwarded to me by Herr Von Mädler. The attempt to construct a section of *Linné* must be considered as exceedingly imperfect, nevertheless it may be of assistance in contributing to more exact observation and measurement at critical periods in the appearance of the crater.

Two facts appear to have been well established lately, viz., the conical form of the object with its shadow, and the early disappearance of the cone and shadow coincident with the appearance of the white spot. The shortness of the shadow in Capt. Noble's drawing, combined with the Sun's altitude at *Linné*, indicates, I apprehend, that the cone has a very slight elevation above the surface. Assuming both drawings to be moderately accurate as to the proportion of western slope and length of shadow, a simple calculation shows that, taking the length of shadow in Mr. Huggins' drawing = 1·000, the height of the highest part of the cone was on the 26th of June, 0·014, Sun's altitude = 0°46′·5. By measurement *on the drawings* the shadows were as follows, Huggins ·21, Noble ·17 parts of $\frac{1}{2}$ an inch. Tacchini's drawings do not exhibit exterior shadows. Noble's when reduced in the proportion of the drawings = ·12. Now, assuming *no* error in the drawings, nor alteration in the height of the cone, we ought to have had on the 22nd of October with a solar altitude of 1° 7′·69226 as the length of the shadow, that of June 26th being equal to 1·00000. Upon comparing the shadows (reduced to the same scale) that on October 22nd falls short of what it should have been by ·10199, or slightly more than a tenth part. At present it is quite out of our power to decide upon this difference as resulting from errors in the drawings, or measurements from them, or from change in the height of the crater, but it indicates the course we should pursue, viz. to observe the times of disappearance and reappearance of the shadow, also those of the appearance and disappearance of the white spot.

The combination of the drawings by Messrs. Huggins, Noble, and Tacchini, appears to elucidate in some degree the phenomena of the white spot. As soon as the Sun attains an elevation above the horizon equal to the inclination of the eastern slope of the cone, the shadow disappears. This was observed on June 26th, but no observation of the disappearance of the shadow on October 22nd has come to hand. We cannot, therefore, construct a section for October 22nd. Taking in the drawing of Mr. Huggins the length of the shadow = 1·00000, and the Sun's altitude at disappearance of shadow = 1° ± we can find the extent of that part of the base of the cone from the perpendicular passing through the west peak of the small crater to the extremity of the east slope = ·77497. For the determination of the inclination of the west slope we need the altitude of the Sun at the first appearance of the shadow to the west. Most observers agree that the small crater is west of the centre of the white spot. It is therefore probable that the west slope has a greater inclination than the east, and constructing the section to exhibit this we have ·77497 from the west peak to the east base, + ·36000 from the west peak to the west base, = 1·13497 for the approximate diameter of the base. This cannot be received as *accurate* from the absence of data having reference to the west slope. Tacchini says, that according to his measures the diameter of the white spot, including the declivity, was in

July or August, 1868, 7″·94. His drawings are accompanied by a scale of 8″, from which it appears that on the 26th of August the diameter of the crater was 1″·9. With these data we obtain the following dimensions of the features observed by Tacchini, viz. the crater and a small dark spot in the middle of the white spot. This dark spot he regarded as a central pit—

Diameter of Crater	=	·35737
West shoulder	=	·09404
Floor ?	=	·15047
East shoulder	=	·11285
Central pit (orifice)	=	·03762

Assuming that the surface around the cone and probably within the crater is strewed with ejected material, more or less fragmentary, resembling *broken glass*, as Mr. Huggins has suggested, we have to a certain extent an explanation of the appearance of the white spot *upon* the disappearance of the shadow. At sunrise the greater inclination of the west slope will cause it to appear as a bright, well-defined spot; the east slope being very gradual, the sun soon attains an altitude greater than the inclination of the slope, and the bright spot becomes merged in one of more than double its diameter, immediately upon the disappearance of the shadows, which is very rapid. The observation of the disappearance of the shadow after sunrise and its reappearance before sunset will assume some degree of importance, as any change in these phenomena with regard to solar altitudes will indicate corresponding changes in the surface around the orifice. On the other hand, if the surface remain in the same quiescent state the shadows will disappear and reappear with the same solar altitudes.

J. H. Mädler to W. R. Birt.

"*Bonn, October 7th*, 1868.

"Dear Sir,—The joined copy of a letter from the parts of Mr. Tacchini, received 22nd September, with the original drawing accompanying it, will be, as I believe, a precious document regarding the crater *Linné*. Therefore I have the honour to send you both of them.

"I take this occasion to remark, that the great whitish spot surrounding *Linné*, which I have found in the English observations and in the drawing of Mr. Tacchini, is never seen by me in 1831. The crater was surrounded by the greenish colour of the *Mare Serenitatis*.

"We have finished the translation of the preface of my work of the Moon, which expresses clearly my opinion about its contents. I shall send it in the next to Mr. Goodwin, to look it over on account of the language, adding the request to send it to you directly. If only the meaning is clearly understood we think to go on with the translation."

M. Tacchini to Professor Mädler.

"*Reale Osservatorio, Palermo, 16 Sept.* 1868.

"Dernièrement j'ai fait des observations sur le cratère *Linné*, et je me permets de vous en envoyer quelque détail, et même des dessins, que vous pourrez confronter avec les votres, puisque ce n'est qu'à une autorité aussi respectable que la votre qu'il peut être donné de juger sur les prétendus changements du cratère.

"Dans le nuit du 26 Juillet j'ai observé l'ombre intérieur du cratère, comme l'indique la fig. 1. Dans le 28 les conditions atmosphériques étaient encore bonnes, et dans le cratère je n'ai plus observé l'ombre aussi marquée, mais dans la partie occidentale on observait seulement une trace d'une légère pénombre, qui donnait encore l'indice d'une faible cavité, fig. 2; en profil le *Linné* serait donc comme la fig. 3, tandis que les autres cratères environnants avaient encore une ombre bien nette et noire. Dans le 29 le cratère apparaissait complètement blanc, alors j'ai porté à conclure que le cratère n'est plus dans les conditions enrégistrées par vous.

"Dans la lunaison suivante j'ai répeté mes observations, et j'ai été encore plus heureux. Dans la nuit du 24 Août le *Linné* était à peine sorti de l'ombre, voyez fig. 4. Dans le 25 le cratère était bien distincte, et le contour bien marqué à l'occident, et presque invisible dans la partie opposée. Dans le 26, avec beaucoup de surprise, j'ai vu encore la cavité de *Linné*, mais plus restreinte, et semblait une petite tache a demi-noire dans le milieu d'une tache blanche, fig. 5, mais jamais une ombre nette comme dans les autres cratères voisins, le terminator passait par *Timocharis*. Ce petit cercle sombre ne correspond donc pas à l'espace renfermé par le contour du cratère, mais il est un creux central; alors le profil de *Linné* serait comme la fig. 6. La tache blanche toute entière, c'est-à-dire avec le peute extérieure, serait selon mes mesures $7''.94$.

"Dans votre lettre au R. P. Secchi vous dites que le cratère montrait l'ombre de l'intérieur encore, quand les autres cratères environnants en étaient dejà depourvues. Donc le cratère a changé véritablement de forme, du moins dans l'intérieur.

"Eh bien, d'après cela, je dois vous adresser une prière, peut être sera trop hardie, mais j'espère d'être favorisé pour le bien de la science.

"Pourriez-vous m'envoyer une copie de vos dessins? Je les publierais en lithographie, et alors la question sera, je crois, definitivement resolue, car avec des simples mots il est toujours difficile de s'entendre.

"Agrécz, Monsieur, mes salutations bien distinguées,

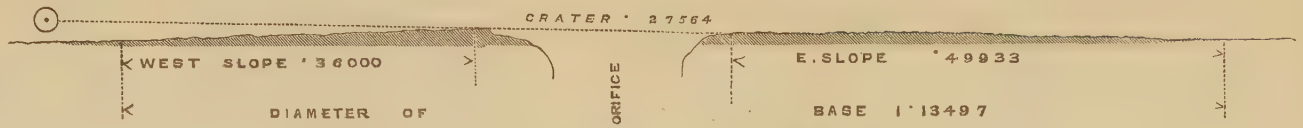
"Tacchini Pierre,

"*Astronome adjoint de l'Observatoire de Palermo.*"

Section of *Linné*, from a Drawing by Mr. Huggins, 1868, June 26.

Length of Shadow = 1·00000.

Sun's Altitude at disappearance of external Shadow.



Section of *Linné* from Drawings by Mr. Huggins and Prof. Tacchini.

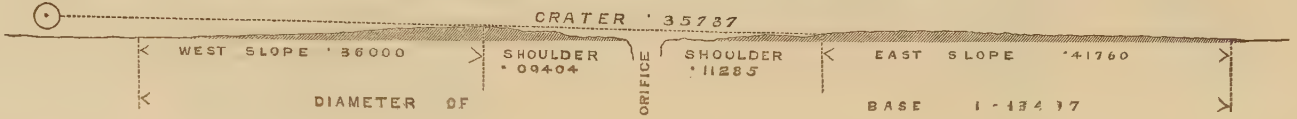
1868, June 26 and Aug. 26.

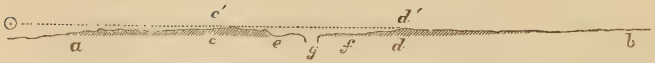
Measure of White Spots 7″·94.

Height of Cone, $\frac{14}{1000}$ of Shadow.

Sun's Altitude at disappearance of external Shadow 1° ±.

Length of Shadow = 1·00000. Sun's Alt. 0° 46′·5.





Section deduced from drawings by Messrs. Huggins, Noble and Tacchini, and a measure by Mr. Buckingham on June 26 Length of shadow = 5″·75. Sun's altitude, 1° ±.

	English miles.	English feet.
The Cone—		
Height of west rim *c c′*	..	492
Height of east rim *d d′*	..	265
Base *a b*	7·8	41282
West slope *a c*	2·4	13094
Crater *c d*	2·5	12998
East slope *d b*	2·9	15159
The Crater—		
West terrace *c′ e*		3420·4
Floor *e f*		5472·9
East terrace *f d′*		4104·5
The Orifice—		
The central pit of Tacchini *g*		1368·2

Line of Section east and west nearly.

The dotted line from ⊙ represents the line of the Sun's rays at the time when the shadow on the exterior towards the east disappears.

Although the preceding deductions are presented not only in the form of, but are actually the results of, observation, and not taken from an *imaginary* section, yet they must be received as approximations only, serving rather as a guide to observers than as being expressions of the *exact* state of *Linné* at the present epoch. It is nevertheless probable that within certain limits the features above specified will be found normally correct.

Elements of New Minor Planets. By Professor Watson.

I have determined the orbits of the new minor planets lately discovered by me, and the results are the following,—

Elements of Hecate (100).

Epoch = 1868, Sept. 1·0, Washington M.T.

M =	10° 5′ 30″·4	
π =	304 45 0·5	Mean Equin. 1868·0.
Ω =	128 28 37·7	
i =	6 33 34·6	
φ =	8 39 32·5	

$\log a = 0{\cdot}493331$

$\log \mu = 2{\cdot}810010$

$\mu = 645''{\cdot}669.$

Elements of Helena (101).

Epoch = 1868, Sept. 13·5, Washington M.T.

M =	17° 48′ 53″·0	
π =	328 40 51·0	Mean Equin. 1868·0.
Ω =	343 35 0·1	
i =	10 4 19·5	

$\log a = 0{\cdot}410460$

$\log \mu = 2{\cdot}934317$

$\mu = 859''{\cdot}640.$

Elements of Planet (102).

Epoch = 1868, Oct. 12·5, Washington M.T.

M =	33° 43′ 52″·6	
π =	326 15 4·8	Mean Equin. 1868·0.
Ω =	135 56 56·4	
i =	5 21 35·2	
φ =	4 37 37·6	

$\log a = 0{\cdot}431728$

$\log \mu = 2{\cdot}902415$

$\mu = 798''{\cdot}758$

Elements of Planet (104).

Epoch = 1868, Sept. 13·5, Washington M.T.

M =	316° 9′ 43″·2	
π =	62 11 55·4	Mean Equin. 1868·0.
Ω =	43 46 42·1	Mean Equin. 1868·0.
i =	2 53 26·7	Mean Equin. 1868·0.
φ =	11 22 53·3	
$\log a$ =	0·502401	
$\log \mu$ =	2·796405	
μ =	625″·756.	

Elements of Planet (105).

Epoch = 1868, Oct. 12·5, Washington M.T.

M =	106° 13′ 49″·8	
π =	242 36 17·8	Mean Equin. 1868·0
Ω =	187 54 1·8	Mean Equin. 1868·0
i =	21 38 59·0	Mean Equin. 1868·0
φ =	10 8 54·0	
$\log a$ =	0·376567	
$\log \mu$ =	2·985156	
μ =	966″·398.	

Elements of Planet (106).

Epoch = 1868, Oct. 10·5, Washington M.T.

M =	346° 44′ 32″·5	
π =	35 37 53·6	Mean Equin. 1868·0.
Ω =	62 42 38·9	Mean Equin. 1868·0.
i =	4 41 33·2	Mean Equin. 1868·0.
φ =	11 14 46·0	
$\log a$ =	0·505287	
$\log \mu$ =	2·792076	
μ =	619″·550.	

Ann Arbor, 1868, *Nov.* 23.

Observations of Jupiter's Satellites. By J. Joynson, Esq.

			h m s	
1868, Sept. 1	I.	Sat. I. first contact	10 39 55	G.M.T.
		last do.	10 45 45	
8	II.	Sat. Oc. R. noted	9 57 31	
		last contact	10 1 25	
	I.	Shad. I. noted	11 39 25	
		full on	11 43 39	
	I.	Sat. Tr. I. first contact	12 23 8	
		last do.	12 29 24	
	I.	Sat. Tr. E. last contact	14 36 17	
Oct. 10	I.	Sat. Tr. I. first contact	8 9 26	
		last do.	8 15 47	
	I.	Sat. Tr. E. first contact	10 18 13	
		last do.	10 23 28	
	I.	Shad. E. first contact	10 32 41	
		last do.	10 36 6	
11	I.	Sat. Ec. Reappearance	7 47 59·9	
17	II.	Sat. Oc. D. first contact	7 44 34	
		last do.	7 52 24	
	III.	Shad. E. first contact	7 58 30	
		last do.	8 8 10	
	I.	Sat. Tr. I. first contact	9 52 13	
		last do.	9 57 18	
	I.	Shad. I. noted	10 17 23	
		full on	10 20 45	
	II.	Sat. Ec. Reappearance	11 4 11·1	
	I.	Sat. Tr. E. partly off	12 7 18	
		Interrupted by cloud.		

Waterloo, near Liverpool.

ERRATA.

Monthly Notice for November 13, 1868.

Page 8, line 19 from bottom, *for* same contact, *read* some contact.

— 12, — 5 from bottom, *for* $22^h\ 2^m\ 35^{s}{\cdot}7$, *read* $21^h\ 2^m\ 35^{s}{\cdot}7$.

— 15, Observations by H. J. Carpenter, *for* 12^h, *read* 21^h.

— 17, line 16 from top, in reference to the phase of the egress observed by Mr. Dunkin, *for* figs. (2) and (3), *read* figs. (1) and (2).

CONTENTS.

	Page
Fellows elected	33
On the Preparatory Arrangements which will be necessary for efficient Observation of the Transits of *Venus* in the years 1874 and 1882, by the Astronomer Royal	ib.
Remarks by Capt. Richards	43
,, Capt. Toynbee	45
,, Rear-Admiral Ommanney	ib.
,, Staff-Commander J. E. Davis	46
,, Mr. Stone	47
On the Observations of the Transits of *Venus* by means of Photography, by Mr. De La Rue	48
Ephemeris for Observations of the Physical Features of the Planet *Mars* during the Opposition of 1869, by Mr. Marth	53
Description of Drawings of *Mars*, by Mr. Joynson	56
Observations of the Transit of *Mercury*, Nov. 4, 1868:—	
,, by Mr. Browning	57
,, by Mr. Talmage	ib.
,, by Mr. Knott	58
Description of a new Chronograph Pen, by Mr. Ellery	ib.
Observations of the Meteoric Shower of November 13–14, 1868, made at the Glasgow Observatory, by Prof. Grant	60
Meteors of November 13–14, 1868, by Prof. Kirkwood	62
The Lunar Crater *Linné*, by Mr. Birt	63
Elements of New Minor Planets, by Prof. Watson	69
Observations of *Jupiter's* Satellites, by Mr. Joynson	71
Errata	ib.

Printed by STRANGEWAYS and WALDEN, Castle St. Leicester Sq., and Published at the Apartments of the Society, Jan. 4, 1869.